UNIVERSITY OF CALIFORNIA

DEPARTMENT OF BACTERIOLOGY
BERKELEY 4, CALIFORNIA

April 25, 1950

Dear Joshua –

First, physical plans. I am teaching summer session at the University of Washington from June 19 – July 19, and plan thereafter to spend a little time climbing in the Grand Tetons with a couple of friends. However, I should get back to Berkeley during the first week of August. This program is conditional on my still being a member of the U.C. faculty at that time, which is by no means certain. I've been well to the front of the battle with the Regents over the Loyalty Oath issue, and this political problem has taken a large part of my time during the past academic year. The settlement finally attained is one of which I deeply disapprove, and I haven't made up my mind whether to accept it and stay or go elsewhere. If the latter, I'll probably be busy moving at the end of the summer. So I can't promise anything definite at present.

Have you heard from our housing bureau yet? They have your name listed, and should be beginning to come through with rental possibilities. If you don't receive any information from them during the next couple of weeks let me know, and I'll see what can be done in the way of direct action (ad. in the local paper).

Are you planning to go to the ~~SAB~~ meetings? I'll be there, and will probably know more clearly by then about ~~them~~ my own future. ~~It~~

Second, scientific matters. With reference to your query re paper II, I suspect that rapidly-dried cells are still in large measure viable, but we have never determined this, since our primary interest at that time was to get cells that would NOT behave like living ones, and could thus be used to study the various individual step-reactions. This is something that deserves systematic study but hasn't yet received it. My work has been pretty much at a standstill for the last 7-8 months as a result of heavy teaching + loyalty rot.

I don't know what to make of your K-12 mutant which forms a constitutive galactosidase; particularly its substrate responses seem most peculiar. On the other hand, the finding that K-12 on rich media can be "pre-adapted" in the absence of specific substrates doesn't frighten me too much. It could mean that precursors + available energy are present so abundantly that the adaptive lag is reduced to the point of no longer being experimentally observable. This is the orthodox explanation, of course! There is naturally the alternative that the enzymes are actually produced in the absence of the specific substrates. It might be instructive to find out whether the presence of DNP has any effect on the rate of attack when added before the specific substrate. If the former explanation is correct, there might be a detectable rate-difference over a period of 60 minutes or so between treated + untreated cells. If the latter, there should be none.

Am very busy just now, so will write no more. I hope you'll be in Baltimore so that we can talk over some of these matters.

Best wishes

Roger